silence by Mr. Newcomb; but it is important. It would be difficult to select from such groups of observers—the French, for example, who saw no connecting ligament—those who saw "the phenomena exactly as we know they are;" and unless this can be done, I am afraid that Mr. Newcomb's somewhat unique argument upon this point might be made as easily to prove the converse as the result he deduces from it. All these observations, in my view, are good; but they are not strictly observations of the same phenomena.

Mr. Newcomb rejects at once the force of the evidence of the observers of the transits of Venus, 1761 and 1769, upon the question of the connecting ligament "till we have better evidence than now exists that their object-glasses were such as Clarke or Foucault would call good." The phenomena connected with the ligament must be far more marked in the case of Venus than in that of Mercury, on account of the large diameter of Venus. To reject therefore by an impossible condition all the evidence in our possession respecting transits of Venus is certainly a bold step; but Mr. Newcomb appears to me to attach far too much importance, so far as irradiation phenomena are concerned, to the improvements effected in modern telescopes. The image of a point of light on the most perfect object-glass which can be conceived is not a point, but a disc, of which the illumination degrades rapidly from the centre, and which is surrounded by concentric rings of light. The law of degradation of the illumination of the central disc has been given by me in the Monthly Notices, November 1865. The result of theory upon these points has been most completely tested by experiment. The existence and regularity of these concentric circular rings is one of the most delicate tests of the perfection of a telescope. Since we have a disc of light corresponding to a point in the most perfect object-glass which can be made, the visible image of the sun formed by such a glass will not terminate with the geometrical image. This result of theory is confirmed by experiment. The optical enlargement found under degrees of illumination similar to those very commonly adopted in observations of the sun is amply sufficient to produce by its destruction near the point of contact the phenomena which so many observers of experience have declared that they have seen. That the optical enlargement is sufficient for the purpose can be seen from the experiments of Dr. Robinson, of Armagh, and from the Greenwich discussions of eclipse observations. This was pointed out in my letter in your number of August 24. With respect to Mr. Newcomb's remark as to the application of this theory of irradiation to a transit of a planet, viz., "we require to know whether the irradiation of an extremely minute thread of light darkened so as to be barely visible is the same with that of a large disc, I am decidedly of opinion that it is not, and if not, the fact that the sun's disc is optically enlarged by the telescope or the eye of the observer cannot be directly applied to the phenomena of transit." I have merely to remark that Mr. Newcomb is undoubtedly right when he asserts that the irradiation from the minute thread of light darkened so as to be barely visible is not the same as that of the large disc. *It is simply because such is the case that the phenomena of the connecting ligament appear.* When the planet is well on the disc, the irradiation around the disc will not be disturbed, but as the planet approaches the edge, the irradiation near the point of contact must eventually be disturbed, and this disturbance, or change, gives rise to the phenomena observed—a black drop, connecting ligament, or whatever name you prefer to give to that apparent cutting out of a piece of the sun's edge near the point of contact which must take place. After the disturbance of the irradiation has once commenced, the connecting ligament must at egress increase in breadth; but I do not profess to be able to give the law of the changing form with any degree of exactness.

The experiments of Wolf and André were, as I stated in my letter of August 24, made upon a disc presenting no sensible traces of optical enlargement. The results can therefore have no bearing upon the question of irradiation. These results are undoubtedly valuable in themselves, as showing experimentally the tendency of errors of observations of contacts under feeble illumination. They may throw light upon those observations at which no connecting ligament was seen, but they are useless to disprove or prove irradiation effects.

My authority for stating that the observations of Wolf and André were made upon a disc showing no sensible traces of optical enlargement, is contained in the memoir itself. If Mr. Newcomb is pleased to call the phenomena of "telescopic irradiation" a species of bad definition, there can be no objection on my part to his doing so; but it is not a species of bad definition "which disappears in a fair atmosphere, with a good telescope well adjusted to focus."

With respect to the ligament not being a celestial reality. The contact is not a celestial reality. My views of the reality of the phenomena are that the reality is neither more nor less than the reality of the phenomena presented at the focus of an object-glass when turned upon a star. The irradiation can in my view be got rid of to the same extent and in the same manner that the central disc corresponding to the star's image can be got rid of. You can reduce its dimensions by cutting down the illumination, and the disc will become a point, but only as it vanishes.

If I may be allowed to give one word of warning respecting the preparations for the transit of Venus 1874, it is *uniformity.* Make such arrangements as you think best, but once made stick to them even if not absolutely the best. The observations which are to be compared must be made as early as possible under the same optical conditions. The whole success or failure of the work will, in my opinion, turn upon the extent to which this necessary condition is approximated to.

E. J. STONE

Royal Observatory, Cape of Good Hope, Nov. 18, 1871

The Origin of Insects

WITH your kind permission I will answer Dr. Beale's questions, published in his letter in your issue of December 21, 1871.

Dr. Beale asks me what part of the nervous system of the maggot is present in the fly? My answer is that I have traced the changes of the one directly into the other; and that Weismann has done the same. There is no time in the pupa state when the nervous system is absent; but it is difficult to demonstrate this, as amongst so much molecular matter it is not easily found, and is very easily crushed and destroyed.

Again, Dr. Beale asks me if I have seen any vestige of the mouth organs of the imago in the larva? I reply that the mandibles and maxillæ exist in the egg twelve hours before the young maggot emerges, together with the fore and hind-head segments; that these have all disappeared when the egg hatches; but that the imaginal discs are already formed at that time. Now, I would ask if it bears the slightest aspect of probability that the larval head segments and mandibles, maxillæ, &c., are formed for nothing, and that the imaginal discs are new formations arising contemporaneously with the disappearance of the larval head segments? Dr. Weismann has shown unmistakably that the abdominal segments of the pupa skin are formed from the abdominal segments of the larval skin. Does it appear in the slightest degree probable that the thoracic and head segments have a totally dissimilar origin? I admit that I have not been able to see the imaginal discs in contact with the head segments of the embryo; but I have found the imaginal discs immediately after the egg is hatched, and they are then too much like the embryonic structures alluded to, to have had any other probable origin. The proboscis is formed from cells laid down within these discs; of that there is not the smallest doubt. Dr. Weismann makes the same assertion, and, although I did not know it to be so at the time I wrote my work on the fly, I acknowledge it is so now, and that in my description of the origin of the proboscis I was wrong. In the Lepidoptera, and in some beetles, imaginal discs may be seen to have their origin in the inner layer of the larval skin.

Again, Dr. Beale says:—"Does Mr. Lowne mean to say, for instance, that he or any one else can adduce any reliable observations to prove that the sexual organs are gradually developed, even from the time when the embryo is enclosed within the egg?' I answer, yes. My own observations confirm those of Weismann on this head, and Dr. Beale will find, on looking again at page 112 of my book on the blow-fly, that he has not correctly quoted my statement. I will also refer Dr. Beale to Dr. E. Bessel's paper, "Studien über die Entwicklung der sexual-Drüsen bei den Lepidopteren," in the "Zeitschrift für wissenschaftliche Zoologie," vol. xvii. I believe future observers will find the sexual organs are always so formed, even as they are in the vertebrata. There is another paper, by Siebold I think, on the same subject in the above-quoted periodical.

Lastly, Dr. Beale asks me to explain what I mean by the sentence occurring at page 116 of my book:—"All the tissues of the larva undergo degeneration, and the imaginal tissues are redeveloped," &c. I apprehend that the redevelopment of all the

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tissues does not imply also the redevelopment of the insect. That the tissues are all so redeveloped is undoubted, but they are not all redeveloped at once. I have stated in my book again and again that certain organs are redeveloped in a particular manner, and was never under the impression that the whole was a case of alternate generation. I did not know the origin of the imaginal discs in those days.

With your permission I will add a few words in support of the assertion "that the pupa change is analogous to ordinary ecdysis, of which it is a modification." In ordinary ecdysis the muscles undergo degeneration at their points of attachment to the cast skin; in metamorphosis this change is far more marked. In ecdysis in Chloëon, for instance, Sir J. Lubbock (Linn. Soc. Trans., vol. xxiv.) has shown that the wings and thorax are gradually developed through nine successive sheddings of the skin. In the more remarkable metamorphosis of Lepidoptera they are developed in two ecdyses, these two being called metamorphosis. Prof. Owen believed, and the assertion is now widely known, that the larvæ of such insects as the Orthoptera, Neuroptera, &c., exist in the maggot form in the egg; but the observations of Mr. Newport on Meloe, and of Fritz Müller, of Weismann, and many others, go far to prove that this is not so—that the maggot form is intermediate, the half-developed embryo and the pupa or perfect insect, being most alike.

The subject is one of great interest, and therefore I trust you will excuse this long trespass on your pages.

99, Guilford Street BENJAMIN T. LOWNE

In Re Fungi

YOUR sarcastic correspondent "F. L. S." is quite incompetent to reply to my former letter. I did not call in question the correctness of the determination of *Agaricus cartilagineus*, but merely drew attention to the absurdity of the statement that the said determination was made from a mere "mass of mycelium," and that such a statement should come from a journal specially devoted to Botany.

In the original report of the occurrence of *Agaricus cartilagineus* (*Journal of Botany*, vol. iii. p. 28) special reference is there made to the "many-headed pileus;" now some of these "pilei" (not the "mycelium," "F. L. S.,") were forwarded to the Rev. M. J. Berkeley for examination, and from *these materials* he (and not the writer of these lines) made out the plant to be *A. cartilagineus*. Certainly I included the species "without hesitation" in the list of Middlesex Fungi, because I knew the plant referred to had not been determined from a mere "mass of mycelium," but that Mr. Berkeley had examined the perfected parts.

I fail to see why "F. L. S." is so anxious to "allay my alarm as to the decay of Fungology in England," especially as I have never expressed any "alarm" on that head. I do not look upon the *Journal* as such an infallible weathercock as to connect its wrong statement with a national breakdown in Botany; neither do I see how I have "missed the point" of its paragraph. I am more inclined to think that I have *hit it* in a friendly way, and rather hard too. W. G. S.

Mr. Baily on Kiltorkan Fossils

IN your last number Mr. Baily is said to have brought forward at a meeting of the Geological Society of Dublin "some strong facts to prove that the Irish palæontologists had not misled Prof. Heer, as stated by Mr. Carruthers at a recent meeting of the London Geological Society."

At the meeting referred to, Prof. Heer placed the Irish beds at the base of the Carboniferous series, mainly because *Sagenaria Veltheimiana*, a coal measure plant, was found in them.

Into this error I said "Prof. Heer had been led chiefly by the erroneous determination of the Kiltorkan *Lepidodendron* by the Irish palæontologists." I will not burthen your columns with the strange history of the nomenclature of this plant, as I shall have an opportunity of doing this elsewhere ere long. The point before us is this, that Mr. Baily alone has the credit of erroneously determining the Kiltorkan plant to be the same as an already described Carboniferous species. And the proof of this is easily adduced. In 1864, Mr. Baily, in his "Explanation of Sheets 187, &c., of the Irish Survey," figures the fossil, and describes it unhesitatingly as "*Sagenaria Veltheimiana*, Sternb. sp." This he repeated in a paper by the lamented Prof. Jukes in 1866 (*Journ. Geol. Soc. Ireland*, i. pp. 123, 124), as well as in a paper by himself read to the Natural History Society of Dublin in the same year (p. 2). Prof. Heer acknowledged his obligations to Mr. Baily for the Irish specimens he had examined. I have examined specimens so distributed by Mr. Baily, and they were named *Sagenaria Veltheimiana*.

In the volume of the British Association Reports, published in 1869, Mr. Baily says (p. 59) that the *Sagenaria* is named by Schimper *S. Bailyana*. More recently (Nov. 1871), in his "Figures of British Fossils" (p. 84), he names it *Knorria Bailyana*. It is not much to the purpose to say that it is neither a *Knorria* nor a *Sagenaria*, or further that the specific designation *Bailyana* must give place, with some dozen other synonyms, to the original name given by Dr. Haughton in 1855. But it is to the purpose to notice that *Sagenaria Veltheimiana* is *not* a Kiltorkan fossil, though said to be so by Mr. Baily, and that this error, now acknowledged by Mr. Baily himself, was the main foundation of Prof. Heer's argument.

I am not a little curious to know what are the "strong facts" which will overthrow a plain narrative that fully justifies my statement, but at the same time compels me to make it more personal than the truth seemed to me to demand when I made it some months ago. WILLIAM CARRUTHERS

ZOOLOGICAL RESULTS OF THE ECLIPSE EXPEDITION

A STEAMER is eminently unqualified for observations on marine zoology. Owing to the high rate of speed, it is impossible to use a towing net with any success, and to a zoologist it is perfectly tantalising to see swarms of Medusæ, &c., sail past the ship without being able to obtain a single specimen. In Peninsular and Oriental ships the only practicable method is to keep the tap of the baths constantly running through a fine gauze net. In this way quantities of Entomostraca may be obtained. Since we have been in the Red Sea, the water has been splendidly phosphorescent every night, the light being most brilliant where the hot water from the condensers is shed out into the sea, the animals being probably killed by the heat, and emitting in the act one last brilliant flash. If the water be turned on into one of the baths at night, most gorgeous flashes of light are obtained, and the animals causing them may be caught in small vessels and kept for examination. They are at present almost exclusively Entomostraca of the genera *Cypris*, *Cyclops*, and *Daphnis*. When the light is examined spectroscopically, it gives a spectrum in which only the green and yellow are present, the red and blue being sharply cut off. Several species of the Entomostraca obtained contain a brilliant red pigment, which gives unfortunately no absorption bands when examined with the micro-spectroscope. At Suez I obtained a number of Echinodermata of the usual dark purple tint, a splendid *Comatula* in abundance, two species of *Echinus*, and one or two star-fishes. The colouring matter of these animals is readily soluble in fresh water or alcohol, as is that of the common British feather-star. Though its colour is extremely intense, it gives no absorption bands, but when a strong solution is used, the spectrum is reduced to a red band, all the rest of the light being absorbed. Apparently parasitic on a large flat *Spatangus*, were obtained a number of *red* Planarians, about one-eighth inch long, which gave the characteristic absorption bands of hæmoglobin with great intensity. The existence of hæmoglobin in Planarians is a fact of considerable interest, and I believe quite new. On taking a boat excursion round the shores, where I obtained abundance of large Gasteropods and the Echinodermata mentioned above, I was remarkably struck by the absence of Actinias. Though I was out nearly the whole day, I did not see a single specimen, nor indeed did I observe any large Medusæ. This absence of these latter may perhaps, however, have been due to the set of the wind or tide.

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